" I do not think that any country in the world has ascertained all its mental defectives. That would be the ideal, but I think it will never be realized.

" But I think Denmark is the country which has ascertained the largest number of defectives.

" The total number of mental defectives being under *care* in Denmark is about 8,700, or, with 3·5 millions of inhabitants, about 1 in 400 of the population.

" The approximate frequency of mental deficiency in Denmark will, I think, be *just the same* as in England, or about 8 per 1,000. This would give for Denmark about 28,000 mental defectives, and of these one-third are already under special care."

Mrs. Hodson has, then, represented Denmark as having about one quarter as many mental defectives as are estimated to exist in that country. Denmark, in fact, closely approximates to this country in the proportion of mental defectives to its total population.

I am, Sir,
Yours truly,
C. P. BLACKER.

Protest (I)

To the Editor, Eugenics Review

SIR,—As I have written to you already twice, I should be glad to hear why Mr. Hill, in his review of my *Vererbungslehre* (No. 3, 1936, of the EUGENICS REVIEW), regards my ideas on theoretical questions of heredity as " vague " and " of little value as scientific hypotheses," and why he blames me for " jumping the most dangerous fences of facts and logic." Such strong words ask for an explanation, this the more as quite a lot of critics of prominent authors stand in sharp contrast to the opinion of Mr. Hill.

PROF. DR. L. PLATE.
Zoologisches Institut und Phyletisches Museum, Jena, Germany.

*** Dr. Plate's statement that he has already written to us twice upon this matter needs qualification. His previous communications were not intended for publication in the REVIEW; they were personal letters to the Editor.

To the Editor, Eugenics Review

SIR,—I am grateful for the opportunity of giving reasons for some of my criticisms of Dr. Plate's text-book of genetics.* For the sake of brevity, I shall confine myself to citing a few typical examples and leave it to readers familiar with Dr. Plate's writings to add to this list if they so desire.

A useful beginning is Dr. Plate's " Erbstock " hypothesis, for this demonstrates his method of dealing with facts and logic. As pointed out in the review of the first volume, Dr. Plate postulates the existence of an " Erbstock " which is supposed to be responsible for the fundamental features of organization, such as symmetry relations and the formation of organs. This " Erbstock " is located in the nucleus but not in the chromosomes, and hence it does not segregate. The ordinary genes in the chromosomes are alleged to cause only the differences between organs, not the organs themselves. Chromosomal genes therefore determine long or short, flat or curled, dusky or transparent wings of a Drosophila, but not the question of " wing or no-wing." If, Dr. Plate argues, there existed segregating genes for the organs themselves, one would expect that mutations of these genes would lead to the loss of whole organs. This he tells us, does not occur. Furthermore, he asks, if the organs are determined by ordinary genes, why do not these genes sometimes so mutate as to give rise to a race with one organ substituted for another? This again, Dr. Plate assures us, does not occur.

Even if the facts were in agreement with this argument, Dr. Plate's point would not be proved. There are good reasons for the view that every organ depends on many genes, and indeed Dr. Plate himself considers this possibility (p. 932). It follows that if a single gene for an organ mutated, the result would not necessarily be a loss of the corresponding organ, but often only a structural anomaly, and if there were a loss this would not always be complete.

But the facts do *not* accord with Dr. Plate's argument. There are quite a number of genes which suppress whole organs, and Dr. Plate himself enumerates some of them. In Drosophila, for instance, there exist eyeless, apterous, aristaless, and ovaless (lack of ovary) genes. An unprejudiced geneticist might conclude that such genes make the assumption of an " Erbstock " unnecessary. But not Dr. Plate. If his hypothesis is to survive, these examples must be explained away—not apparently a difficult matter. Since the manifestation of some of these genes is irregular, it is concluded that it is not the inactivation of genes which cause the organs, but the appearance of " inhibiting " factors which only secondarily interfere with the activities of those genes (in the " Erbstock ") that are the true builders of the organs. This is, to say the least of it, far-fetched. For if an " inhibitor " exists, obviously it inhibits only incompletely. Why should the " real " gene not be incompletely inactivated? To make things worse, some of these genes have a complete and regular manifestation, such as apterous and ovaless. And what is the function of the normal allelomorphs of the inhibitors? If they do not cause the normal development of the organs concerned, are they without any function? That seems unlikely.

A logical analysis shows that the whole hypothesis is dialectical jugglery made possible only by the vague use of the term " inhibitor." For what is an

* EUGENICS REVIEW, 1936, XXVIII, p. 129 and 226.

" inhibitor " ? If we say that the gene apterous " inhibits " wing formation, this clearly is only a restatement of fact. We can say that blue eyes " inhibit " brown eye-colour, or that brown eye-colour " inhibits " blue ; both these statements are equally true and equally meaningless, for they only say in different words that there exist two different phenotypes. They say nothing about the genes concerned. The term " inhibitor " acquires a specific and precise meaning when applied to *known* units, and only then is it more than a restatement of fact. An example will make this clearer. Purple eye-colour in Drosophila is due to a recessive gene, wild-type eye-colour (red) being dominant. Now there exists a purple-inhibitor which suppresses the action of purple ; a fly homozygous for purple but containing the purple-inhibitor in addition has red eyes. But the fact that this gene is an inhibitor for purple can only be concluded from the observation that purple itself is due to a gene. The term " inhibitor " has a meaning only in relation to the *known* gene which is inhibited, just as the term " modifier " has only a meaning in connection with the gene whose action is being modified.

From this it is evident that Dr. Plate applies the term " inhibitor " to the genes eyeless, apterous, etc., as though the genes in the " Erbstock " which he wishes to demonstrate were already demonstrated. If so, he is arguing in a circle. Or else, he is using the term " inhibitor " only in reference to phenotypes, in which case it is only a restatement of facts.

As to organ substitutions, there are several of these and they are all known to and mentioned by Dr. Plate. The most striking cases in Drosophila are proboscipedia (mouth-parts footlike), tetraptera and bithorax (balancers turned into a second pair of wings), and aristapedia (segmented foot with claws and pulvillus instead of a feathered bristle, on the antenna). Aristapedia impressed even Dr. Plate (p. 1108), but he does not admit that these cases disprove his " Erbstock " hypothesis. Instead, he hints at the irregular manifestation of these genes. In view of the multifactorial basis of most structures, this should not be surprising. How could a suitable genetic background for these organs already be present ? Modifiers ensuring a uniform arista could not possibly—barring a miracle—be expected to do the same job in connection with the substituted foot. Irregularity of manifestation is therefore to be expected after such a radical structural change has taken place. Anyone who has seen aristapedia flies must wonder what kind of facts would convince Dr. Plate !

Having dealt briefly with organs, one may turn to the symmetry relations of the body. Snails are twisted either clockwise or counter-clockwise. The direction is usually constant within a species or family, but inversions occasionally occur. In Vol. I, p. 56, Dr. Plate writes as follows :

> " Welch found in Bundovan in Ireland about 2,000 fossil sinistral *Tachea nemoralis*. Inversion can therefore also occur on a hereditary basis and is then in my opinion caused by a mutation of the ' Erbstock ' (blastovariation) by which the structure of the cytoplasm of the egg is altered."

In the interval between the appearance of the first and second volume, Boycott and Diver showed that in *Limnæa peregra* dextrality and sinistrality are caused by a single pair of allelomorphs. One might now have expected Dr. Plate to admit defeat. Far from it. He declared (Vol. II, p. 934) that " in reality spiral cleavage is the feature of organization, and whether the twist occurs to the right or to the left is a racial character. . . ." As long as the case is unanalysed, it is used as an argument for the " Erbstock " hypothesis ; as soon as the analysis has declared against that hypothesis, the significance of the case is denied!

Even if we follow Dr. Plate's new argument and assume that only the cytoplasmic structure is caused by the " Erbstock," but not the direction of the pattern, we are at once faced with formidable difficulties. In animals there is no case in which a symmetrical species has been crossed with an asymmetrical one, or in which representatives of bilateral symmetry can be crossed with radially symmetrical types. There is, however, a case in plants. In the snapdragon, *Antirrhinum majus*, the ordinary flower is bilaterally symmetrical, but in addition there exists a recessive gene which produces flowers with a beautiful radial symmetry. Manifestation of this gene is regular, and the plants are healthy and vigorous ; in short, it is the critical test case for Dr. Plate's new standpoint. He knows of it, for the gene appears in a table on page 202, is described in some detail on page 334, and is again mentioned on page 1103. Strangely enough, it is not mentioned in connection with the " Erbstock " hypothesis.

It is hardly credible that Dr. Plate should have overlooked the importance of this striking case ; and looking for possible reasons why he may have doubted its significance, I can think of nothing except that what is concerned is only a racial character and not the feature of a higher systematic unit. This would be in conformity with his views on the *Limnæa* case. If this guess is correct, the argument is at an end. For if racial characters are discredited, we are left with those cases which *in principle* cannot be analysed. For Dr. Plate himself admits (p. 933) that the presence or absence of genes can be demonstrated only from segregations. Segregation can only be obtained from fertile hybrids, and here even the species barrier is usually unsurmountable. One must conclude that all the obtainable evidence is against the " Erbstock " hypothesis ; and if Dr. Plate now resorts to evidence which in principle is unobtainable, he cannot expect us to follow him there.

To sum up. The facts disprove Dr. Plate's " Erbstock " hypothesis—one of the corner-stones of his speculations on evolution—and his attempts to explain these facts away are based on faulty

logic. The criticism that has been applied to the "Erbstock" could be extended to others of Dr. Plate's speculations, but is there any point in refuting speculations which in a period of twenty years have utterly failed to justify their existence?

H. G. HILL.

London, W.C.1.

Protest (II)

To the Editor, Eugenics Review

SIR,—I trust you will allow me to reply to some of the many misrepresentations made by Mr. Cecil Binney in his review of my book, *Sacrifice to Attis: a Study of Sex and Civilization* (October 1936, p. 234).

Mr. Binney says that I have "printed a most misleading list of great men . . . to show that religion did not appeal to those who had known want." I never said anything so demonstrably absurd. What I said was, "Poverty and humble parentage have not prevented the attainment of the highest positions in statesmanship, arms, law, science, art, exploration and commerce, but singularly few with these disadvantages have attained eminence in the Church." This is a widely different thing, and I gave a list to support my statement.

Mr. Binney also says that I "attribute homosexuality both in males and females to the father complex." Actually I have not discussed the causes of homosexuality in the male, and (following most psychologists) I attribute it in the female to persistence of strong mother-love, increased in some cases by subconscious identification with the father.

Finally Mr. Binney says that my history is at fault because, when making a remark obviously intended to apply only to a modern English audience, I make no reference to Greek or Elizabethan drama. He charges me with inaccuracy at the end of his review but has not quoted a single inaccurate statement of mine, and has shown very inaccurate reading of my book himself.

WILLIAM A. BREND.

14 Bolingbroke Grove,
S.W.11.

To the Editor, Eugenics Review

SIR,—In reviewing a book such as Dr. Brend's, one must for the sake of brevity attempt to summarize without quoting at length the author's views; but I do not think that, in doing so, I have misrepresented him. Dr. Brend challenges me to indicate inaccuracies in his book. Let me start with his list of great men, who rose from "poverty and humble parentage." Among them are included Columbus, Cabot, Drake and Frobisher, who were all of the merchant-adventurer class; Cobden, who was son of a farmer, and Cervantes, who was not of particularly humble origin. "Early familiarity with the hard, real facts of life is not conducive to piety," comments Dr. Brend on his list, although, of those mentioned, Livingstone was a missionary, Arthur Henderson was a leading member of his own sect, and Lincoln was a deeply religious man. Again, he omits to notice that throughout the Middle Ages many of the great ecclesiastics came from the humblest origins; even in the present century there has been one peasant Pope.

Dr. Brend remarks: "The apocryphal books of the New Testament contain as good history as the Gospels," which is absurd to anyone who has read them. He says of the word σταυρος, "Liddell and Scott's dictionary gives the first meaning of this as 'an upright pale or stake.' It seems probable, therefore, that this picture was in the minds of those who first wrote the account of the Crucifixion." It seems less probable when one discovers that this original meaning of σταυρὸς is cited by Liddell and Scott from Homer's Odyssey. He says of Lot's daughters: "Their conduct is not condemned, and the daughters became the founders of well-known tribes"—whereas the whole point of the story is to furnish a revolting origin for the bitterest enemies of the Israelites. Again he says: "Mary's status was not lowered by the fact that her subsequent children were the offspring of Joseph"—a very doubtful assumption; and again, "if children could be allowed simply to crawl about with the cattle as they did in the time of Tacitus," without any indication that Tacitus' reference is to the supposed condition of the "happy savage" outside the empire; and again: "Lesbianism is derived from the homosexual practices of the poetess Sappho," without mentioning that there is no evidence that Sappho ever did anything of the sort.

I observe that Dr. Brend says that his observations on the drama are limited to the modern English drama, but in that case they seem out of place in a presumably general discussion of "sex psychology of women."

CECIL BINNEY.

7 King's Bench Walk,
Temple, E.C.4.

The Female Sex Cycle

To the Editor, Eugenics Review

SIR,—I was greatly interested in Dr. Zuckerman's study on the Physiology of Fertility in the April number of the REVIEW, and specially in his conclusions as to the viability of sperms and ova in the human subject. There has recently come to my knowledge the case of a woman whose only coitus of the month was on the day before she expected to menstruate. To her surprise, no menstruation occurred and pregnancy resulted. Such a case raises questions.

If Dr. Zuckerman's opinion of the very short viability of the human ovum after ovulation, or of the sperm after immission be true, it would be necessary to account for such facts as the above in some way or other. The first, and most obvious,